

SCIENCE

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A CURE FOR TETANUS AND DIPHTHERIA.¹

THE greatest interest has been aroused in scientific circles in Berlin by a paper in the *Deutsche medicinische Wochenschrift*² by Behring and Kitasato. These well-known bacteriologists, who for a long time past have been working in Dr. Koch's Hygienisches Institut, have not only succeeded in producing immunity against diphtheria and tetanus, but also in curing animals already infected by these diseases. Their results are to a great extent self-explanatory, and there is every reason to expect that the same method will be found to be applicable to other infectious diseases. The most remarkable part of their discovery is the fact that the blood of an animal that has been made immune against diphtheria possesses the extraordinary power of destroying the poison formed by the microbe of this disease. This power is also possessed by the serum of such an immune animal, which serum can therefore be used as a curative means on other animals that are suffering from this disease. The same statement holds good for tetanus.

Before describing in detail these interesting results, it will be well to give a short historical review of some recent bacteriological work which can be regarded as having led up to this discovery.

Towards the end of 1888, Nuttall,³ working in Flugge's laboratory at Breslau, discovered that various bacteria are destroyed when mixed with fresh blood or blood-serum, and, further, that this destruction cannot be ascribed to the action of cellular elements, but rather to the fluid part of the blood. This discovery (which really arose from the German criticism of Metschnikoff's phagocyte theory) was soon followed by the work of Buchner⁴ and Nissen⁵ on the bacteria-killing power of the cell-free blood-serum. These authors considered that their work necessitated a limitation of the phagocyte theory, and suggested a new view of the nature of immunity, whether natural or acquired. In other words, they suggested that immunity was conditioned by the bacteria-killing power of the various body fluids rather than by that of any particular kind of cell. These opinions were rather severely criticised in a paper by Lubarsch⁶ that was published towards the end of last year. Lubarsch emphasized the fact that while the serum of the rabbit—an animal extremely sensitive to anthrax—has a great power of destroying anthrax bacilli, horses' serum has no such power,

although this animal is comparatively refractory to the disease. Again, while on the one hand such eminently pathogenic microbes as the anthrax and cholera bacilli are capable of being destroyed by serum from various animals, several perfectly harmless microbes find blood-serum to be an excellent food-medium. Further, though the serum of the rabbit kills anthrax bacilli in a pre-eminent degree, the living blood-plasma of this animal can only do so to an infinitesimal extent. Such considerations suggested to Lubarsch that the bacteria-killing power of the blood-serum was a fact rather of the nature of an epi-phenomenon than an essential factor in the conflict between the organism and the microbe. In May of this year appeared my own work on "Defensive Proteids."¹ I gave this name to a new class of proteid bodies, which I found to possess a bacteria-killing power, and which I have obtained from the spleens and lymphatic glands of various animals. This work has a distinct bearing on the foregoing, in that it suggests that the bacteria-killing power of blood-serum is due to minute traces of these substances liberated for the breaking-down of lymphatic cells. The absence of a bacteria-killing power from certain kinds of serum (e.g., horse) and from living blood-plasma (as has been shown for that of the rabbit in regard to anthrax) appears to be connected with the intactness of the leucocytes in these special cases. Further, the fact that I obtained these substances from cells which either are or can become phagocytes, may be taken as an additional proof of Metschnikoff's well-known theory. These substances appear to be absent from the normal blood-plasma, or, at any rate, only present in such small quantities that they cannot be separated from it. With blood of febrile animals, however, the case is different, and from such blood I have been able to isolate a bacteria-killing substance.² This fact appears to indicate that these substances are actually used by the organism in its re-action against the attack of pathogenic microbes.

During last summer, while I have been engaged in this work, various other papers have appeared, which tend to show still more clearly that the bacteria-killing power of the blood-serum (or, if my work be accepted, of defensive proteids) is of real importance in the production of immunity. Bouchard³ was, I think, the first of many authors who have succeeded in showing that the bacteria-destroying power of blood-serum from immune animals is greater than that of normal serum. Bouchard proved this in the case of bacillus

¹ From Nature, Dec. 11, 1890.

² No. 49, Dec. 4, 1890, p. 1113, "Ueber das Zustandekommen der Diphtherie-Immunität und der Tetanus-Immunität bei Thieren."

³ "Experimente über die bakterienfeindlichen Einflüsse des thierischen Körpers" (Zeitschrift für Hygiene, vol. iv. p. 353).

⁴ "Ueber die bakterientödtende Wirkung des zellenfreien Blutserums" (Centralblatt für Bakteriologie, vol. v. p. 817, and vol. vi. p. 1).

⁵ "Zur Kenntniss der bakterienvernichtenden Eigenschaft des Blutes" (Zeitschrift für Hygiene, vol. vi. p. 487).

⁶ "Ueber die bakterienvernichtenden Eigenschaften des Blutes und ihre Beziehungen zur Immunität" (Centralblatt für Bakteriologie, vol. vi. p. 528).

¹ "A Bacteria-killing Globulin" (Proceedings of the Royal Society of London, vol. xlviii. p. 93), and "The Conflict between the Organism and the Microbe;" Part 2, "On Defensive Proteids" (British Medical Journal, July 12, 1890).

² "Indications of a Method of Curing Infectious Diseases," read at the Leeds Meeting of the British Association for the Advancement of Science, September, 1890.

³ "Sur l'effet des produits sécrétés par les microbes pathogènes" (Paris, 1890).

pyocyaneus for rabbits. He made the animals immune by injections of sterilized culture-fluids, and found that serum from such animals exerted a far greater "bactericidal" action on the microbe in question than serum from a normal animal. Behring and Nissen,¹ in a paper published in May of this year, went a step further. They showed, that, whereas blood-serum from an animal made immune against anthrax exerted an increased bactericidal action on the anthrax bacillus, it showed no increased action on the bacillus pyocyaneus. Conversely blood-serum from an animal made immune against the latter microbe had no increased action on the anthrax bacillus, though it exerted a powerful bacteria-killing action on pyocyaneus. The authors considered that they had proved the existence of two bodies, each having a specific action on one of the two microbes in question, and, further, that these substances are present in animals made immune against the above-named diseases. These remarkable conclusions acquire a still greater interest when received in the light of a research by Gamaleia published at the beginning of last year.² Gamaleia found that the aqueous humor of a sheep, about three days after inoculation with attenuated anthrax, acquires bactericidal properties for this microbe. This condition lasts for nearly a month, and then gradually vanishes, though, as is well known, the sheep remains immune for a far longer period. These researches therefore suggest, first, that, when an animal has been made immune against a pathogenic microbe, its blood and other body fluids contain a substance capable of destroying the microbe in question; second, it follows that such protective substances can remain in the body undestroyed for a considerable time; and, third, that they can be present in such quantities as to be able to kill the microbes involved (even without the help of living cells) and yet produce no appreciable ill effect on the general health of the animal. If this is so, why should it not be possible to cure any infectious disease by injecting a "lymph" obtained from the blood or tissues of an animal previously made refractory to the disease in question?

Whether or not the above considerations stimulated the researches of Behring and Kitasato, their work affords a positive answer to this question, which promises to be of the greatest importance to humanity, and has led them to the most unexpected and interesting results from the scientific standpoint. The following is a summary of their paper, which is of the nature of a preliminary communication.³ The method by which, in the first case, they produced immunity against tetanus and diphtheria, is not described. Only so much of their results is communicated as is necessary to support the following propositions:—

"The immunity of rabbits and mice against tetanus depends on the power possessed by the fluid part of their blood of rendering harmless the poisonous substances produced by the tetanus bacilli."

This proposition involves a completely new theory of the nature of acquired immunity. Hitherto it has been thought that immunity must depend either on the voracious activity of phagocytes, or on the above-mentioned bacteria-killing power of the blood, or on an acquired tolerance against a poison; and, further, that by the method of residues any one of these theories could be proved by showing the other two to be false.

Behring, however, was able to prove, by his work on diph-

theria, that none of these theories would account for the natural immunity of rats or the artificially produced immunity of guinea-pigs against this disease. After disproving many speculations on this subject, the above-given explanation was arrived at, but they only obtained a satisfactory proof of its correctness when they began to test it on the tetanus microbe.

Their experiments prove,—

(1) That the blood of rabbits which have been made immune against tetanus can destroy the tetanus poison.

(2) This character can be shown to be possessed by the blood both before and after it has left the vessels, and in the cell-free blood-serum obtained from it.

(3) This character is of so permanent a nature that it is still manifested by such serum after it has been injected into other animals: consequently, by transfusion of such blood or serum, important therapeutic actions can be obtained.

(4) This power of destroying the tetanus poison is absent from the blood of such animals as are not immune against tetanus; and, after such animals have been killed by the tetanus poison, it can be shown to be present in their blood and tissues.

In support of these assertions the following experimental results are brought forward.

A rabbit was made immune against tetanus by a method which will be described in a forthcoming paper by Kitasato in the *Zeitschrift für Hygiene*. To prove the completeness of its immunity, 10 cubic centimetres of a virulent culture was injected into it. Half a cubic centimetre of the same culture was quite sufficient to produce tetanus in a normal rabbit. The treated rabbit, however, remained immune, and it showed immunity not only against the tetanus bacillus, but also against the poison produced by this microbe, for it remained unharmed by an injection of twenty times the quantity of tetanus poison which will kill with certainty a normal rabbit. Blood was taken from the carotid artery of this rabbit. Before clotting occurred .2 of a cubic centimetre of this blood was injected into the body-cavity of a mouse, and .5 of a cubic centimetre into that of another. Twenty-four hours later, these mice, together with two control-mice, were inoculated with tetanus of such virulence that the latter showed the symptoms of tetanus after twenty hours, and were dead in thirty-six hours. Both of the treated mice, on the contrary, remained healthy.

The greater quantity of the blood of this rabbit was allowed to stand, and its serum collected.

Six mice each received 2 cubic centimetres of this serum in the abdominal cavity, and all withstood a subsequent inoculation with tetanus. Control-mice died of tetanus within forty-eight hours.

With this serum the authors succeeded in curing animals that had been previously infected with tetanus. They have also been able to show that this serum possesses an intense power of destroying the tetanus-poison.

Of a ten-days-old tetanus culture which had been sterilized by filtering, .00005 of a cubic centimetre was enough to kill a mouse after four to six days, and .0001 would always produce the same result in less than two days.

Five cubic centimetres of the serum of a tetanus-immune rabbit was mixed with 1 cubic centimetre of this filtered culture, and kept for twenty-four hours. Of this mixture, four mice each received .2 of a cubic centimetre (that is to say, .033 of the original culture, or more than 300 times the quantity which would otherwise be capable of killing a mouse).

¹ "Ueber den bakterienfeindlichen Einfluss von verschiedenen Serumarten" (*Zeitschrift für Hygiene*, vol. viii, p. 412).

² "Sur la destruction des microbes dans les corps des animaux fébricitants" (*Annales de l'Institut Pasteur*, 1889, p. 229).

³ A fuller account will shortly appear in the *Zeitschrift für Hygiene*.

All these four mice remained in good health. Control-mice, on the contrary, which were at the same time inoculated with .0001 of a cubic centimetre of the original culture, succumbed within thirty-six hours.

All the mice mentioned in each of the above series of experiments have been subjected to repeated injections with the tetanus bacilli, and have shown themselves to be permanently and completely immune.

This result is the more remarkable in that up till now, in spite of innumerable attempts, no one has ever succeeded in making any animal whatever immune against tetanus. A theory of the nature of acquired immunity which at once led to a method of treating the disease which is easy to understand, harmless to the animal, and certain in its effect, must surely possess some basis in fact.

Naturally every kind of control experiment with serum of normal rabbits has been carried out with uniformly negative results. Serum of cattle, horses, and sheep has also been found to have no action on the tetanus poison. The living blood and tissues of an animal which has not been made immune, likewise show no power of destroying the tetanus poison, as appears from the following experiment, which has been many times repeated:—

Rabbits into which .5 of a cubic centimetre of a germ-free tetanus culture is injected subcutaneously, succumb after showing typical tetanus symptoms. Almost always a serous transudation is to be found in the thoracic cavity. Of this transudation .3 of a cubic centimetre is, on the average, enough to kill a mouse with typical tetanus symptoms. The same is true for the blood.

The authors close their paper by pointing out the possibility that their method of curing tetanus and diphtheria which they have used with such brilliant results on animals so highly susceptible to these diseases as mice and rabbits, may also be used for the far less susceptible hospital patient. They also note the possible influence of their work on the practice of blood-transfusion.

E. H. HANKIN.

HEALTH MATTERS.

Risks to Health in East Africa.

THE colonizing wave setting steadily from Europe to East Africa gives peculiar interest to Dr. Kohlstock's experience of the risks to health and the chances of longevity among his compatriots in that region. As director of the sanitary arrangements at the German headquarters, says the *Lancet*, he has had excellent opportunity of forming his opinions, and the sense of responsibility with which he gives them to the world is in some measure a guaranty of the care with which he has collected his facts and drawn his conclusions. The first note he strikes is one of warning. Let no one, in any stage of phthisis, even the pre-tubercular, think of settling in East Africa, if he does not want to leave his bones in its soil. At first this danger was not appreciated in the Fatherland, and the inspection of officers setting out with colonizing parties was carried out in somewhat perfunctory fashion. But the climatic conditions of the region soon made their effects apparent, and nine subalterns had to be sent home, — precisely those in whose families pulmonary phthisis had prevailed. For a man of thoroughly sound constitution the two diseases to be dreaded are dysentery and malaria. The former, in Dr. Kohlstock's experience, responds satisfactorily to the measures usually taken in European centres in the East, the disease among the German troops running generally as favorable a course as in French or English garrisons. The latter is dangerous only when the patient is precluded from taking rest, and compelled to continue at work; as, for instance, on necessarily forced marches. Even so, but three fatal cases have as yet been recorded among the German troops in East Africa as due to

malaria. As a rule, under conditions of rest the malaria patient soon gets well. In stubborn cases he has to be transferred to the sanatorium; the transference hitherto being effected on ship-board, in the absence of railways. Very often the change of locality, coming after the voyage, has sufficed to restore the patient's health. A liberal allowance of fresh butcher's meat has proved the most efficacious diet in malaria: indeed, the risks arising from the disease have been greatly reduced by the excellent nursing and accommodation now enjoyed by the patient. Dr. Kohlstock holds it to be a mistaken practice to completely cut off alcohol as a prophylactic against malaria: he would rather, within the limits of temperance, that the German in East Africa should live, as far as possible, as he did at home. The necessary upturning of the soil for purposes of tillage is, in such virgin territories as that of German Africa, the most prolific source of malaria; and, at that inevitable stage of colonizing operations, the sanatoria must be in constant requisition, and their treatment supplemented by change of locality for the convalescent. So well, however, have these measures been understood and carried out, that Dr. Kohlstock can point to a steady diminution in the statistical returns of malaria cases; the places where the disease has been most pronounced being, naturally enough, those like Mpwapwa, where the earth exhalations from the disturbance of long inert soil have been the most extensive, while no good water-supply has been obtained by boring. Soon, however, a marked reduction of the malaria returns will, it is hoped, be effected even in that locality.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

The editor will be glad to publish any queries consonant with the character of the journal.

On request, twenty copies of the number containing his communication will be furnished free to any correspondent.

A New Kansas Meteorite.

THE year 1890 has brought to scientific knowledge a larger number of tangible celestial visitants than all preceding years combined. Up to this year the Waconda meteorite was the only representative from Kansas on the list of authentic meteoric falls. In March of this year the now famous group of irons from Kiowa County was made known to science; and on June 25, 1890, the Washington County aerolite was heard and seen to fall at mid-day by thousands of Kansas citizens; and now, just at the close of the year, I have the pleasure of announcing a third fall of unknown date. This may be called the Tonganoxie meteorite. So far as now known, this fall consists of a single specimen, weighing 26½ pounds. It is an iron of the ordinary character (not a pallasite). It is of an irregular shape, and is thought by the owner to resemble a lion couchant. It is 9½ inches long, 6½ inches wide, and 3½ inches deep.

This meteorite is the property of Mr. H. C. Fellow, principal, 1887-90, of the Friends' Academy at Tonganoxie, in Leavenworth County, now pursuing a post-graduate course of study in the University of Kansas. Mr. Fellow bought it in the spring of 1889 of Mr. Quincy Baldwin, who found it upon his farm, one mile west of Tonganoxie town, in 1886. Mr. Baldwin was not aware of its true character, although he had manufactured a fish-hook from a small fragment of the iron. He considered it to be a piece of iron ore, and proposed to start an iron-mine upon his farm; but this fragment proved to be the only "indication," and the mining project was reluctantly abandoned. This meteorite is now deposited in the museum of the University of Kansas, but is still the property of Mr. Fellow. A preliminary analysis shows the presence of iron, nickel, and cobalt. Professor E. H. S. Bailey will soon publish a complete analysis.

A small portion of the surface has been polished, and exhibits very distinctly the Wiedmanstaaten figures. Careful search has recently been made for other fragments of this meteorite on the Baldwin farm and vicinity, but without success.

F. H. SNOW.

Lawrence, Kan., Dec. 27.

Dr. Hann's Studies on Cyclones and Anticyclones.

PROFESSOR FERREL's letter in *Science* of Dec. 19, commenting on mine of May 30, closes with the suggestion that I should make further statement of the matter of Dr. Hann's studies, which I do with pleasure.

The best reasoned general account of the convectional theory of cyclones and anticyclones (by the latter term I mean areas of high pressure) that I know of is given in Professor Ferrel's "Popular Treatise on the Winds." Of various statements in regard to cyclones, the following may be quoted from the concluding paragraph on their vertical circulation: "The greater temperature of the interior [of cyclones] causes an upward expansion of the air and greater vertical distances between the isobaric surfaces here than in the exterior part where the temperature is less" (p. 241). In regard to anticyclones or areas of high pressure, of the kind that Dr. Hann has investigated, the following explanation may be quoted: "The principal cause of the large areas of very high barometer which frequently occur in the higher latitudes in winter is undoubtedly found in the clearness of the atmosphere over these areas and the intense coldness produced by the radiation of heat at a time when little is received from solar radiation. The density and pressure of the air are much increased from this cause, and the areas are too large and irregular for this disturbance to give rise to a cyclone with a cold centre" (p. 345). The inversion of temperature accompanying such areas of high pressure is referred to on the next page, but still with the implication that the mass of air in the anticyclone is cooled below the temperature of the surrounding atmosphere, and therefore that it descends and flows out at the base by gravitative convection.

These quotations might be further extended, but they suffice to show that the essential of the generally accepted theory of the areas of low and of high pressure which appear so frequently on our weather-maps is that the first are relatively warm, and the second are relatively cold, when compared with their surroundings. Cyclonic and anticyclonic areas are both of common occurrence, and therefore as a rule their temperatures should be respectively above and below the normal temperatures of their time and place.

Records of temperature made on high mountain-peaks furnish the best means of testing the convectional theory of cyclones; for, even if all other tests were successfully borne, failure under this test would be fatal to the theory. Dr. Hann's essay on the anticyclone of November and the cyclone of October, 1889, as observed in the Alps, furnishes the best means of applying this test that has come to my knowledge. It is true that one example of each of these phenomena is not sufficient for final determinations, and it is very apparent that the results would be far more convincing if they included records from mountain stations scattered over a much larger area than that of the Alps. Surely no one will be more careful to supplement these deficiencies, whenever possible, than Dr. Hann himself.

I do not see any reason for believing that the anticyclone that stood over the Alps in November, 1889, was exceptional in its nature or in its relation to the surrounding atmosphere. All of its features except its mean temperature warrant the belief that it was a typical example of the phenomena referred to under the heading of "Areas of High Pressure" in Professor Ferrel's treatise. Unless it can be shown to have been of exceptional nature, the abnormally high temperature of its air mass is a direct contradiction of the fundamental idea of the convectional theory of areas of high pressure. It has not been claimed that the conditions of a cyclone exist in this high-pressure area; but the explanation of high-pressure areas as quoted above is a direct corollary of the cyclonic theory. If the corollary is contradicted by facts, the theory needs revision. The burden of proof in this case lies with those who would maintain that the anticyclone in question was of so exceptional a nature that it cannot be regarded as a representative of its class. Its long duration does not show it to be a thing of another kind from other areas of high pressure: the long duration merely gave good opportunity for repeated observation of its prevailing high temperature.

As to the cyclone of October that was examined by Dr. Hann, it was certainly of moderate development; but it was as good an example, according to Dr. Hann, as he could find. The observations that he quotes show that its general central temperatures were below the normal of its time and place. The fact that the temperatures were not determined in the free air, but at stations on the surface of the ground, does not seem to me to invalidate their use here; for on the peaks where the critical observations were made the air is generally in motion, and the mass of the mountain is small; and for both these reasons the control of the temperature of the air by the ground is not great enough to explain the reported low temperatures. Over a broad surface of a lowland, where the wind is weaker and the opportunity for contact of air and ground is greater, the case is different. The low temperature of the central part of this cyclone may fairly be regarded as contradictory to the convectional theory of cyclones, unless it can be shown that the example in question was surrounded by air more abnormally cooled than its own, or unless it is shown to have been an expiring cyclone,—one whose long circulation had so thoroughly exhausted its supply of warm, moist air, and so successfully warmed the surrounding air, that it had no further support, as Professor Ferrel has shown might sometimes be the case. It is true that Europe might offer more examples of self-exhausted cyclones than occur in this country, for they are there advancing from moister into dryer regions; but it is difficult to believe that so considerable a deficiency of temperature as probably occurred in the case under consideration should be produced before the cyclonic motions had stopped, if they depended entirely on a convectional origin. It is not likely that so exceptional a case as this must be, if it is to be explained by convection, would have been the very case that Dr. Hann happened to choose for his studies. It is still more unlikely that both the cyclone and the anticyclone here referred to should have been exceptional members of their classes, both departing from the normal in a way that would contradict the convectional theory. As these are the first examples of their kind to be carefully examined by means of regular observations at stations at so high a level, the probability is strongly in favor of their being ordinary, and not extraordinary, phenomena; and as such they did not possess the peculiar temperatures that the convectional theory would lead us to expect. Although mere probability of this kind does not close a case, it seems to me that it may be fairly said to open it.

I do not see that there is any necessary contradiction in this discussion. The theories under consideration are not mutually exclusive. Both may be true. The liberation of latent heat from condensed vapor is an aid to the circulation in both cases. Certainly there is nothing in Dr. Hann's essay to make one think that thunder-storms, tornadoes, and desert whirls are not convectional phenomena. It is entirely possible that true convectional cyclones might prevail in the tropics, while driven cyclones might characterize the temperate zones. A cyclone begun chiefly by one process might be continued chiefly by the other. Of course, this is hypothetical: it was not my intention last May to regard it in any other light. For that reason my letter closed with an "if." Others besides Professor Ferrel, however, understood me to have abandoned the older theory and taken up with the newer. I tried to state Dr. Hann's point of view, and I do not regret having stated it so fairly that it was taken for my own. That I had not adopted it as fully as Professor Ferrel implies, may be inferred from the close of my eighth paragraph and from the middle of the ninth, as well as from the ending of the letter already referred to. But in making this explanation, I do not wish to be understood as not welcoming the new theory. The abnormal warmth of anticyclones had been in my mind as a difficulty in the way of convection, yet I had expected that cyclones would be found to be still warmer; and it was not until reading Dr. Hann's forcible statement that I perceived I had become too strongly settled in favor of the prevailing theory. On recognizing this partiality, I made all the more effort to give full and fair consideration to the new one. It seemed to me nothing less than a duty to announce the facts and Dr. Hann's interpretation them in the same journal that had published my outline and

ing of the other theory some years before; and, in spite of Professor Ferrel's letter, it still seems to me that I was right in saying that the convectional theory needs revision in the light of Dr. Hann's results, but by revision I do not mean abandonment.

The incompleteness of the new theory is not a reason for being silent about it. It should be welcomed, if only for the reason that it will cause a healthful revision of previous views. The value of multiple working hypotheses has been so well set before our scientific readers, that nothing more need be said on that point. I will not venture to speak for Professor Ferrel, but I am sure that practically every meteorologist in the country will profit from a serious re-examination of his knowledge of the theory of cyclones in the light of Dr. Hann's researches.

As to the process by which the general circulation of the atmosphere shall produce cyclones and anticyclones, it is not to my mind necessary that this should be worked out completely before the suggestion of it may be profitably made. But it does not seem impossible that the general winds might here and there crowd together, owing to irregularity of flow; that, where crowded together, anticyclones would appear; and that, between the anticyclones, cyclonic whirls might be formed. It would be indeed a satisfaction if I could here answer all the pertinent questions, and give all necessary explanations, about such a problem; but, if we may judge by the treatment that dynamical meteorology has received thus far in this country, there is only one American who can do that. I wish that he might consider the possibilities of some such process arising from the general circulation of the atmosphere as is outlined above, and, after working them out rigorously, state them as clearly as he has explained the general circulation of the atmosphere itself. Whatever truth there is in the convectional theory of cyclones would not be harmed by such an investigation, while whatever truth there may be in the hypothesis of driven cyclones would pretty surely be discovered by it.

There is a corollary to the suggestion made by Dr. Hann, that may be of interest to those who seek for an explanation of our past glacial climates. It is generally recognized, that, if there were an increase in the activity of our winter cyclones, there would be an increase of snowfall as well; and, if this were carried far enough, the accumulation of snow might last over the summer. The increase of cyclonic activity would presumably accompany an increase in the general circulation of the atmosphere, if cyclones in our latitudes are driven by the general winds; and this would appear in that hemisphere whose equatorial and polar contrasts of temperature were strengthened. Such strengthened contrasts might be expected in the hemisphere having its winter in aphelion, and particularly at times of maximum orbital eccentricity. I do not mean to imply that a glacial period might depend on this condition alone; yet it may be one of many whose varying combinations at times produce a glacial climate, as Croll and J. Geikie and many others have shown; but this particular element of the combination does not appear to have been recognized.

W. M. DAVIS.

Harvard College, Cambridge, Mass., Dec. 27.

Moisture in Storms.

NEXT to the action of heat in storms, the part that moisture takes in them has been greatly emphasized. The so-called "condensation theory" of storms has had wider acceptance than any other. We may imagine a limited portion of the earth's surface heated up by the sun, and this more or less of a circular shape. There will be induced a tendency to an uprising current of heated air, which will continue so long as the central portion is warmer than the air surrounding it at the same level. This tendency, however, would be quickly brought to rest were it not for the fact that the uprising column has its moisture condensed, which liberates latent heat and causes the column to rise still faster. Here is a most remarkable fact, notwithstanding that the release of this moisture diminishes the total amount in the air, and the latent heat warms up the air, both of which causes would stop precipitation at once; yet we are taught that the force of the storm is increased by this process. There is another serious ob-

jection among many. If rain occurred at the centre of the storm, this theory might be plausible; but since the bulk of the rain in this country occurs three hundred miles to the eastward of the centre, and over only about one-fiftieth part of the area covered by the storm, it requires an enormous stretch of the imagination to grasp the causation of our wide-extended storms through this condensation effect. We may add still another consideration. It is fairly well ascertained that the upper limit of our storms, as shown by pressure and temperature observations at Pike's Peak (14,134 feet), is far above four or five miles, and may extend to the limits of the atmosphere. Now, the bulk of our precipitation is formed within 6,000 feet of the earth's surface: hence it is plain that the condensation of moisture plays a very subordinate part in our wide-extended storms, and has nothing to do with their generation or maintenance.

I do not propose to discuss at this time all the objections to this "condensation theory," which have been repeatedly advanced both in this and other journals, and which have not been answered, but I wish to present a recent most extraordinary abandonment of this theory by Dr. Hann, who stands at the head of the old school on the continent. I quote from a translation, by Professor Blanford of London, of a recent statement by Dr. Hann. Speaking against the condensation theory, he says (*Nature*, Nov. 6, 1890), "These views are such as I have always enunciated (for a long time, indeed, without any apparent result) in opposition to the then prevalent theories of the local origin of barometric minima through the agency of condensing water-vapor (as contended by Mohn, Reye, Loomis, and Blanford). They now begin to make way and prevail. Most clearly is this seen in the case of Loomis, who, in the course of his own persistent study of the behavior of barometric minima and maxima, has been compelled by degrees to give up the 'condensation theory' to which he formerly adhered so strongly, and to ascribe the origin as well as the progressive movement of cyclones to the general circulation of the atmosphere."

The importance of this utterance from such an authority cannot be exaggerated. While I have shown that Dr. Hann has been misled by his study of mountain observations, yet it seems to me this avowal on his part reaches out far beyond that. As I have just shown, the very life and existence of the old theory depend upon condensation of moisture. Now, if Dr. Hann, who must understand this fact most thoroughly, has deliberately set it aside, must we not conclude that it has an inherent weakness in itself to his mind. Those who are familiar with Loomis's work will be surprised to learn that he ever abandoned the condensation theory of storms.

It would seem that this controversy over the condensation theory is rapidly culminating, and the indications point to a speedy downfall of that theory. It is a remarkable fact that all the objections urged against this theory, now these many years, have been studiously ignored; but a few words from a recognized authority, even though based upon a wrong interpretation of facts, seem to make headway very rapidly. Surely Hann, Davis, and Blanford form a most formidable front against this theory, and it is high time its defenders should come to its assistance ere it be too late.

H. A. HAZEN.

Washington, Dec. 13.

["Letters to the Editor" continued on p. 8.]

NOTES AND NEWS.

AT a meeting of the Royal Botanic Society on Dec. 13, as we learn from *Nature* of Dec. 18, the secretary answered various questions as to the destructive action of fogs on plants. He said it was most felt by those tropical plants in the society's houses of which the natural habitat was one exposed to sunshine. Plants growing in forests or under tree shade did not so directly feel the want of light; but then, again, a London or town fog not only shaded the plants, but contained smoke, sulphur, and other deleterious agents, which were perhaps as deadly to vegetable vitality as absence of light. Soft, tender-leaved plants, and aquatics, such as the *Victoria regia*, suffered more from fogs than any class of plants he knew.

—“The Harvard Yard,” an original etching by Robert R. Wiseman, shows the “Harvard Yard,” with a good view of the group of older buildings. The plate is of large size. No plain prints of the etching are to be had for the present, at any rate, possibly not at all. Each *remarque* artist-proof is printed on imperial Japan paper, and bears the signature of the artist and a *remarque* representing the seal of the university, printed in dark crimson. The publishers are the Frederick A. Stokes Company, 182 Fifth Avenue, New York City.

—*Nature* states that a novel whaling expedition is about to be undertaken by three Americans whalers, which have gone to the Arctic regions to winter at the mouth of the Mackenzie River. In order to be well supplied with food, they have taken what will last for two years, and they expect also to get food from the whalers in the summer. This is the highest point any whaler has reached, being a thousand miles from the North Pole. Directly the ice breaks after the winter, the whales come to the mouth of the river in great numbers to feed, and it is expected that a large number of them will be secured.

—A paper by Mr. W. B. Mason in the “Transactions of the Seismological Society of Japan” deserves the attention of all who take special interest in seismology. It contains, according to *Nature* of Dec. 11, a list of earthquakes recorded at telegraph-stations in central and northern Japan from Aug. 11, 1888, to Dec. 31, 1889. Mr. Mason, while allowing for various sources of uncertainty in the observations, thinks that some results may be deduced from what are still meagre statistics. Thus, of the 151 earthquakes recorded in Tokio, only 89 were felt at the other telegraph-stations. Some of those which were felt at all the stations seem to have been felt at almost exactly the same instant: in other words, there was no indication of a progression of the earthquake from point to point.

—Some three years ago MM. Fremy and Verneuil, two French chemists, succeeded in producing rubies artificially. The crystals obtained, says *Engineering*, were small; and since then the inventors have been occupied with the problem of increasing the size of the rubies obtained. To this end considerable changes have been made in their methods of operating. In place of using pure alumina, as in their previous experiments, alumina alkalinized by potassium carbonate is used. This addition of an alkali does not alter the purity of the crystals obtained, while it facilitates their regular formation. In their original experiments the operations were completed in twenty-four hours, but they have now succeeded in prolonging the re-action over several months, with the result of obtaining much larger crystals. As much as seven pounds weight of rubies have been obtained at a single operation. Even yet, however, the crystals are small, but are at least sufficiently large to mount, which was not the case with the first essays of the inventors.

—The curious idea of preserving dead bodies by galvanoplastic method is not new; but we note that a Frenchman, Dr. Variot, has been lately giving his attention to it (*La Nature*). To facilitate adherence of the metallic deposit, says *Nature* (Dec. 18), he paints the skin with a concentrated solution of nitrate of silver, and reduces this with vapors of white phosphorus dissolved in sulphide of carbon, the skin being thus rendered dark and shiny. The body is then ready for the electric bath, which is served by a thermo-electric battery, giving a regular adherent deposit of copper if the current is properly regulated. With a layer of one-half to three-fourths of a millimetre, the envelope is solid enough to resist pressure or shock. Dr. Variot further incinerates the metallic mummy, leaving holes for the escape of gases. The corpse disappears, and a faithful image or statue remains.

—Mr. J. M. Coode records, in the new number of the *Journal of the Bombay Natural History Society*, the following instance of an exceptional method of hunting which the panther is occasionally forced to adopt. Mr. Coode was lately asked by the *patel* of a village in the Amraoti district to accompany him one evening to a forest nursery of young bamboo shoots, to assist in killing a large boar which nightly visited the place and did immense damage. As stated in *Nature*, they waited for some time,

when, just as it was getting dark, they heard the short guttural sound of a panther, and heavy footfall of some running animal. The noises came nearer and nearer, until a nilghai and a panther could be distinctly seen against the sky-line, the former being chased by the latter. The nilghai kept moaning, and was evidently in an abject state of fear. The two ran round in a circle of about one hundred and sixty yards diameter, within thirty yards of where the observers were standing, and passed them twice, both animals making their respective noises. They then disappeared, but Mr. Coode has reason to believe the nilghai got away.

—At the last meeting of the Physical Society (London), as reported in the *Electrical Review* of Dec. 19, Mr. Shelford Bidwell, F.R.S., told a great many useful facts about selenium cells and their behavior; and he gave several experimental illustrations, the most effective of which points to practical applications. Mr. Bidwell connected one of his selenium cells with a delicate relay, which in its turn caused a circuit to be established with an automatic switch and an electric lamp. So long as sufficient light impinged upon the selenium, the electric lamp did not act; but, directly the gas (or daylight in practice) diminished to a certain degree, the electric lamp shone forth in its glory, and again became extinguished when its rival re-appeared. The fact of any light going out could thus be signalled to a distant attendant, and this would be useful in case of ships' lights and numerous other purposes. The effect of different colored glass interposed between the light and the cell revealed peculiar results upon the properties of the selenium, and Dr. Thompson suggested that one could almost imagine the near possibility of seeing by electricity if the effects of colors could be transmitted to distances in some analogous manner.

—It is stated in the “Proceedings of the Royal Geographical Society” (December, 1890) that M. Thoroddsen, the well-known explorer of Iceland, has returned to Reykjavik from his summer excursion into the district between Borgarfjord in the south and Gilsfjord in the north. The topography of the country as shown on existing maps was found to be fairly accurate. The geological results of the journey are more novel. The volcano situated at the extreme point of the peninsula of Snaefellnes was visited. It is especially interesting from the fact that clear indications have been found that this volcano commenced its eruptive activity long before the glacial epoch; and, although no outbreak is known to have occurred within historical time, it is tolerably certain that its activity continued to comparatively modern times. The volcanoes of the district traversed have not the same direction as those in the south of Iceland, viz., from south-west to north-east, but range themselves in a semicircle round Faxa Bay, which is a distinctly volcanic depression. M. Thoroddsen's expedition was largely supported by Baron Dickson.

—Some experiments have just been made at Annapolis by the United States Government with the object of testing the resistance of nickel-steel armor-plates at low temperatures. The plate tested, according to *Engineering* of Dec. 12, which had already received five shots under ordinary conditions, was fired at twice more,—once before subjecting it to a freezing mixture, and once afterwards. A 6-inch gun was used with a powder charge of 44½ pounds, and a Holtzer shell weighing 110 pounds, the striking velocity being 2,055 feet per second. The first shot struck 15 inches from the edge of the plate, and the projectile penetrated till its point entered the wood backing, reaching a distance of 13½ inches from the face of the plate. The shell rebounded, and was picked up entire at a distance of 40 feet from the plate. The plate showed a crack 14 inches long extending down to the left edge of the plate, and another horizontal crack 13 inches long, both of which were apparently through cracks. The plate was then put in a freezing mixture of ice and salt, and its temperature reduced to 28° F. The second shot was then fired, the conditions being similar in all respects to the first. The shell, however, broke up badly, about one half remaining on the plate, and the other half flying to fragments. A triangular piece of the plate, 26 inches across the top, broke off, and was thrown 25 feet in front of the plate. A wide gaping crack connected the hole with

one of the shot-holes previously made in the plate. Numerous old cracks were opened and enlarged, and other new ones made, the longest being 24 inches. With the exception of two cracks, the injury to the plate was in the neighborhood of previous fractures. The perforation of the two rounds was much the same.

— The Swedish expedition to Spitzbergen under the leadership of G. Nordenskiöld and Baron A. Klinkowström returned in safety to Tromsø, as we learn from the "Proceedings of the Royal Geographical Society." The party landed first of all at Horn Sound, whence G. Nordenskiöld made his way on snow-shoes overland to Bel Sound; but the deep snow prevented geological work. The longest stay (July 18 to Aug. 10) was made at Ice Fiord. The farthest point north reached was Lagö, east of Hinlopen Straits. The passage was still quite blocked with ice, and, there being but small chance of being able to penetrate to the Seven Islands, the return voyage was commenced. On their way back, the travellers made hydrographical explorations on the Norwegian islands.

— Professor Brückner of Berne, Switzerland, has recently called attention to the existence of climatological periods of about thirty-five years for the whole globe (more marked in the interior of continents). The years 1700, 1740, 1780, 1815, 1850, and 1880, says *Nature* of Dec. 18, appear as centres of cold, wet periods; while the years 1720, 1760, 1795, 1830, and 1860 are centres of warm, dry periods. During the warm periods the passage of oceanic air to the continent has been hindered, and during the cold it has been favored, increased rainfall occurring in the latter case.

— We learn from *Engineering* of Dec. 12 that Mr. P. Schoop, of the Oerliken Electrical Works (Switzerland), with the object of rendering accumulators more portable, has adopted the plan of absorbing the electrolyte with gelatinous silica. With this object, Mr. Schoop adds a small quantity of sodium silicate to the cell. This is decomposed by the sulphuric acid, and the silica is liberated in the form of a translucent, firm, and elastic jelly, which is unattacked by sulphuric acid, or by the more powerful oxidizing agents which come into existence during the charging. The jelly but slightly increases the resistance of the cell, though it somewhat diminishes its capacity in watt hours. The best method to adopt in gelatinizing a cell is to add to three volumes of sulphuric acid, at a density of 1.25, one volume of sodium silicate at a density of 1.18, and leave the mixture to itself for twenty-four hours. At the end of that time the whole liquid is set to a jelly. In charging a cell, a small quantity of liquid rises to the surface of the jelly, but this disappears again during the discharge.

— The French Government have had carried out for them a number of experiments on gun-steel at very low temperatures. Both hardened and unhardened specimens were subjected to a variety of tests at temperatures of between 75° and 100° below the zero of the Fahrenheit scale. The specimens were cooled, according to *Engineering*, by immersing them in a bath of solid carbonic-acid gas and sulphuric ether, several pounds of the gas being required for this purpose. The first set of tests were simply intended to determine the expansion of the test bars per degree; and the results, though somewhat irregular, showed that the expansion per degree decreases with the temperature. A number of test bars were then prepared in sets of threes, two of each set being used as reference bars, and tested at the temperature of the surrounding air, while the third was cooled down to between 75° and 100° below zero, and then tested, with the following results: both the hardened and unhardened bars had their elastic limit raised by about 11 per cent by being tested cold; the breaking load of the unhardened bars was raised about 3 per cent, and that of the hardened by about 6 per cent, by the cooling; the elongation of the unhardened bars was diminished 12 per cent, and that of the hardened ones 14 per cent; the contraction of area was also less in the bars tested cold. None of these changes are, however, permanent, as the bars completely recovered their original properties on attaining the ordinary temperature of the

air. All the above tests were made in tension in the usual way. For gun-steel, however, the resistance of the metal to shock is of more importance than its strength under a quiet tensile stress. A number of bars were accordingly prepared in sets of threes, as before, and one bar of each set was cooled down to between 75° and 100° below zero, and tested by means of a falling weight, the other bars of each set being tested in the same way at the ordinary temperature. The experiments showed that cooling the bars much increased their brittleness. Thus, on an average, each unhardened bar required 5.9 blows to break it when cooled, as against 14.6 blows for specimens tested under ordinary conditions. With the hardened bars, the reduction in strength was less, 12.57 blows being required as an average at the low temperature, and 14.4 at the ordinary temperature. As before, the metal regained its qualities as its temperature rose. Some further experiments seemed to show that metal into which a great deal of work had been put was less affected by a reduction in temperature, but this requires confirmation.

— According to the *Journal de la Chambre de Commerce de Constantinople*, the greatest electric project which has yet been suggested is being planned,—the construction of a line from St. Petersburg to Archangel. The electric current would be supplied by a series of generating stations distributed along the line. It is estimated that the cost, including the rolling stock, would be 46,509 francs per kilometre.

— *Nature* states that at a recent meeting of the Paris Academy of Medicine, M. Motais of Angers maintained that myopia, or short-sightedness is one of the products of civilization. An unexpected proof of this view was found in the condition of the eyes of wild beasts, such as tigers, lions, etc. M. Motais, having examined their eyes by means of the ophthalmoscope, discovered that animals captured after the age of six or eight months are, and remain, hypermetropic, while those who are captured earlier, or, better still, are born in captivity, are myopic. This short-sightedness is evidently induced by artificial conditions of life.

— On Monday, Dec. 15, Mr. T. G. Pinches read a paper before the Royal Asiatic Society, on the newly discovered version of the story of the creation. He had had the good fortune, in the course of his investigations into the contents of the unregistered tablets in the British Museum, to find in one of them, brought home by Mr. Rassam in 1882, a still earlier version than that which the late Mr. George Smith had translated. It was a bilingual tablet, the text being Akkadian, and the gloss Assyrian; and while the date of the tablet itself was, like the rest of those in Assur-bani-pal's library, not older probably than 650 B.C., the Akkadian text was, in his opinion, an exact copy of an older document, which had, in all probability, been put into its present shape 3000 B.C., or even earlier. One side, the obverse, as described in *Nature*, is devoted to the creation story: the other, the reverse, is simply an incantation form for the purification of the great temple tower E-zida, now so well known as the mound called Birs-Nimrud. The text might be roughly divided into three paragraphs or sections of about ten lines each. The first describes the time when nothing was, neither "the glorious house of the gods," nor plants, nor trees, nor cities, nor houses, no, not even the abyss (Hades) nor Eridu (regarded by the author as Paradise). The second section describes the making of Paradise with its temple tower E-Sagila, founded within the abyss. Then was Babylon made, and the gods, and the land, and the heavens, and mankind. The third section then proclaims the creation of animals, plants, and trees (in that order) of the Tigris and of the Euphrates. The fourth records the building of cities and houses. Of all except the last, Merodach, the god, seems to be the active creator, and he is also to be understood as the builder, through men, of the cities, etc. Mr. Pinches pointed out several interesting words and forms occurring in this oldest form of the creation account, which had subsequently assumed so many diverging shapes. A discussion followed, more especially on the word "Adam," rendered by Mr. Pinches "foundations" (of earth), but by Dr. Zimmern "living things." This was probably the origin of the Hebrew word "Adam."

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LETTERS TO THE EDITOR.

[Continued from p. 5.]

The Subtropical Zones of High Barometric Pressure.

THERE is an old theory, if a mere popular notion which has no scientific basis whatever may be so called, that the two zones of high barometric pressure, extending with a few interruptions around the globe, and having their maxima of pressure about the parallel of 35° in the northern hemisphere, and 30° in the southern, are caused by the crowding of the air, in its passage in the upper part of the atmosphere from the equatorial to the polar regions, into intermeridian spaces, becoming gradually narrower toward the poles. It is supposed that the air, as it is forced into narrower spaces, is turned down toward the earth's surface, and that this descent of the air causes increased pressure on the surface. The barometric pressure in both hemispheres increases from the poles, or at least from some high parallel, toward the equator, until the parallels above mentioned are reached, and then there is a small decrease of pressure to the equator; so that these parallels are simply the limits between the increasing and decreasing pressure gradients in going from the pole to the equator, and the culminating parallels of the convexity of the isobaric surfaces.

The writer's attention was first directed to this feature of these isobaric surfaces about thirty-five years ago, in reading Lieut. Maury's "Physical Geography of the Sea;" and, having no faith in the popular explanation, he made it a matter of study in order to discover the true cause. This was found in the now well-known

law of the deflecting force of the earth's rotation, which was first discovered at that time. By this law the air, in moving from west to east in the middle and higher latitudes, is pressed toward the equator; but, in moving the contrary way in the lower latitudes, it is pressed a little toward the poles, thus causing a bulging-up of the isobaric surfaces with the culminating lines between the two systems of easterly and westerly currents about the parallels of 35° or 30°. The results were published in an "Essay on the Winds and the Currents of the Ocean," which was subsequently republished in "Professional Paper of the Signal Service," No. XII.

Subsequently this whole subject was treated in a more thorough and mathematical manner, and the results were published in a memoir entitled "Motions of Fluids and Solids Relative to the Earth's Surface." This was afterwards republished in "Professional Paper of the Signal Service," No. VIII., with extensive notes by Professor Frank Waldo. In this memoir it was shown that with certain assumed values for the velocities of the easterly and westerly motions of the air, which were quite reasonable and probable from what was known of these somewhat uncertain data, the deflecting force of the earth's rotation would give the observed increase of pressure, on the one hand from the pole, and on the other from the equator; so that there was no room to doubt that the maximum pressure a little above the tropics in each hemisphere was caused by this force. A very full abstract of this memoir was also given in *Silliman's Journal*, January, 1861.

Subsequently this same subject was taken up again, and treated in a more thorough manner and with better data, and the results published in "Meteorological Researches," Part I., "Coast Survey Report for 1875."

The same subject was again treated by the use of mathematical processes somewhat simplified, and given in "Recent Advances in Meteorology," forming Part II. of the "Report of the Chief Signal Officer for 1885."

Finally the whole matter was gone over again by the writer in a popular manner, and explained by means of various simple illustrations, and was given in his "Popular Treatise on the Winds," etc.

Dr. Hann, however, has not accepted the results, nor has he ever attempted to show that they have been deduced from erroneous principles or processes, but has continued to use and uphold the old theory. Not only this, but he has based upon it a new theory with regard to the cause of the high-pressure areas of the middle and higher latitudes. In the *Zeitschrift für Meteorologie* for 1879, p. 39, he first suggests that these regions of high barometric pressure may be simply the places where the upper equatorial and westerly currents settle down towards the earth's surface, as in the case of the zones of high pressure at the polar limits of the trade-winds. His idea is, that as the upper poleward-moving currents in the latter are deflected down by their being crowded between intermeridian spaces, gradually becoming narrower toward the poles, so, even beyond these belts of high pressure, there must be local hinderances, or a damming-up of these currents, by which they pass into descending ones toward the earth, and so cause the high-pressure areas.

In the next volume of the *Zeitschrift* he again refers to this matter, and suggests that the reason why cyclones and great barometric disturbances are more frequent in winter than in summer is that in winter the temperature and pressure gradients of the upper strata of the atmosphere, in a poleward direction, are greater, and hence there is a greater strength of current at this season of the year.

Again, in his "Climatology," published a few years ago, this same old theory is given in explanation of the subtropical zones of high pressure.

Finally, in his recent memoir published by the Royal Academy of Sciences of Vienna, the old theory of the subtropical high-pressure belts is introduced, and also his new theory, deduced from it, of the causes of high-pressure areas; and he refers to his preceding papers in the *Zeitschrift* on these subjects.

Although the teaching of Dr. Hann on these subjects has been entirely at variance with the writer's own views on the same subjects, previously published at so many different times, yet he has

refrained from taking any notice of it. But now that this last memoir has recently been brought to the attention of English, and especially of home, readers, justice to himself requires that this matter shall not be allowed to remain unnoticed any longer.

The question of the cause of the high pressure in the subtropical zones, according to the old theory, is one of the relation between kinetic and potential energy; that is, between velocity and pressure. As the air of the upper part of the atmosphere moves toward the poles, it is supposed to become crowded and checked in its motion, and the kinetic energy changed to pressure. But the question arises as to why this takes place up to a certain latitude only, that of maximum pressure, and not all the way up to the poles; for the maximum velocities of the upper poleward-moving currents must be a little above this latitude, and the converging of the meridians increases up to the pole. As long as kinetic energy is changed to pressure, this must be increased; and so the greatest pressure must be at the pole, and not down at a low latitude. But it may be shown that the whole effect is so extremely small, that it is not worthy of any consideration practically.

The following general expression of the relation between pressure and velocity is taken from "Recent Advances in Meteorology," p. 194:—

$$(1) \quad \log P_0 - \log P = \frac{h}{18401 (1 + .004\tau)} + \frac{s^2 - s_0^2}{360940 (1 + .004\tau)},$$

in which P is the barometric pressure in millimetres of any part of the air with corresponding velocity s ; P_0 equals 760 millimetres, being taken as the value of P at the earth's surface, and the corresponding value of s equals s_0 ; h is the difference of altitudes corresponding to P_0 and P ; and τ is the temperature by the Centigrade scale. If u , v , and x are the meridional, longitudinal, and vertical velocities respectively at any given point, we have

$$(2) \quad s^2 = u^2 + v^2 + x^2.$$

The numerical constants in (1) are adapted to common logarithms, and the expression is strictly applicable to the case only in which τ is constant and in which friction may be neglected.

The first term in the second member of (1), depending upon h , arises from gravity. Where only small portions of air are considered, or strata of very small depths, the part of the pressure depending upon h is so small in comparison with the whole at

air, in moving from the equator, is acquiring increased velocity, there must be a descending pressure gradient; but, as soon as there is a decrease of velocity, there must be an ascending gradient to cause it. The same is true in the lower strata of the atmosphere, where the air returns from the polar to the equatorial regions. The oscillations of the air-particles between these regions are similar to those of a pendulum, in which the force from both sides acts in the direction of the middle point.

With regard to the effect of descending currents, to which Dr. Hann ascribes the local high barometric pressures of the middle and higher latitudes, already referred to, the formula (4) can be applied in this case also. We have only to substitute for u the vertical component of velocity x . This being done, we can readily compute what the value of x must be to give ΔP_0 equal to any assignable value. Let us suppose it is required to find what value x must have to give $\Delta P_0 = 25$ millimetres; that is, an increase of barometric pressure of about one inch. We can, in this case, assume $s_0 = 0$, at least in the middle of high-pressure area. The formula in this case gives $x = 71.2$ metres per second, or about 160 miles per hour, if we put $\tau = 0$ in the formula. For a higher temperature this velocity must be greater.

If any one is disposed to doubt this result given by the formula, let him take the experimental result obtained by Mr. Dines and others, that a velocity of about seventeen miles per hour gives a pressure of one pound per square foot upon a plate exposed at right angles to the current. But the pressure of the whole atmosphere, corresponding to 30 inches of mercury, is about 2,100 pounds. The pressure corresponding to one inch, therefore, is 70 pounds. As the pressure is as the square of the velocity, we must have $x = 17 \times \sqrt{70} = 142$ miles per hour, to give a pressure equal to one inch of barometric pressure. This result is less than that obtained theoretically, because it is well known that the experimental pressure upon a small plate is greater than the theoretical, on account of the effect of friction of the air which passes around

of barometric pressure in the high-pressure belt, above the normal pressure, is about 0.3 of an inch. So the old theory, even upon the extreme supposition that the whole kinetic energy of the upper current is converted into pressure in the high-pressure belt, accounts for only about the $\frac{1}{400}$ part of the observed increase of pressure in this belt. When we consider, then, how small a part of the kinetic energy of the upper current is changed to pressure, and that the most of it passes on to higher latitudes, how extremely small must we suppose the effect from the old theory to be!

Where there is friction, of course some of the kinetic energy is changed into heat, and so the pressure is accordingly diminished; and a little greater velocity would be required to cause the same increase of pressure.

In what precedes we have supposed the kinetic energy to have its origin from some other source than a pressure gradient; but in the interchanging motions between the equatorial and the polar regions, toward the pole above, and the contrary below, this is not the case, but the pressure must decrease from the equator to some middle latitude where the velocity u and kinetic energy are the greatest, and then increase from that to the pole, where it is 0 and the pressure the greatest. The preceding formula is applicable in this case at the equator and the poles, since $s_0 = 0$; and, putting $u = 2$ metres per second, we get $\Delta P_0 = .0194$ millimetres, as before. If we suppose P_0 to be in the latitude where $u_0 = u$, that is, where the velocity of the return current is the same as the maximum velocity u above, then, instead of u^2 in (4), we have $u^2 - u_0^2 = 0$, and hence we get ΔP_0 in this case equal 0; that is, there is no change of pressure here arising from the interchanging motion between the equator and the pole. The pressure, therefore, is a little greater at the equator and the poles than at the latitude where u is a maximum, which, on account of the convergency of the meridians, and the narrowing of the intermeridional spaces, toward the poles, is between the middle latitude and the equator, and perhaps near the parallel of 35° . Instead, therefore, of an excess of barometric pressure here of about 0.3 of an inch, there should be a very slight depression, if there were no other forces to cause this excess. And this is very evident from a very simple manner of considering the matter: for as long as the

depending upon u is so small in comparison with the whole atmospheric pressure, that it may be neglected, and the expression may then be put into the following form:—

$$(3) \quad P_0 - P = \frac{s^2 - s_0^2}{206 (1 + .004\tau)}.$$

This is substantially the same, in different measures and notation, as that of Kaemtz (*Lehrbuch der Meteorologie*, vol. i. p. 150) when used at the earth's surface, where $p' = 760$ millimetres.

In the application of the preceding expressions it is necessary to know the value of s_0 corresponding to P_0 ; but this is known in a few special cases only, since we do not have a complete solution of the dynamic problem of the general circulation, in which the condition of continuity and the frictional conditions are taken accurately into account. It is also necessary to know the streamlines, since P and P_0 must be in the same stream-line.

It is evident from the observations of the cirrus clouds at Zi-kwei (latitude $31^\circ 12'$ north) that the velocity of the poleward moving current of the upper part of the atmosphere at this latitude cannot be more than about two metres per second, or four miles and a half per hour (see *Popular Treatise on the Wind*, etc., p. 122). Let us now suppose that there is a perpendicular wall on the parallel of 35° extending all around the globe, and reaching up to the top of the atmosphere, and that the whole upper half of the atmosphere has a motion, from some cause, directly against this wall, with a velocity u . The current in this case will pass directly down to the earth's surface, where, near the wall, we must have sensibly $s_0 = 0$. Supposing, now, that $P_0 = 760$ millimetres when the whole atmosphere has no meridional component of velocity, and that ΔP_0 is the effect of the upper current: we get from (1), in this case,

$$(4) \quad \log (760 + \Delta P_0) = \log 760 + \frac{u^2}{360940 (1 + .004\tau)}.$$

Putting $u = 2$, and $\tau = 0$, this gives $\Delta P_0 = .0194$ millimetres, or about .00076 of an inch of barometric pressure. The increase

the plate, both upon the air which is retarded and stopped in front of the plate, and also upon that behind the plate.

It is doubtful whether a descending current in the open air of more than two metres per second could be found anywhere in the whole atmosphere. This, we have seen, would increase the barometric pressure 0.0194 of a millimetre, a quantity which could not be detected by the most delicate and accurate barometer. It is seen, therefore, how very improbable is Dr. Hann's theory of the cause of high-pressure areas.

Dr. Hann lays great stress upon the efficiency of the steep gradients of the upper part of the atmosphere, in the middle and higher latitudes, in producing both cyclones and high-pressure areas. But the forces arising from these gradients are almost completely counteracted by the deflecting forces of the earth's rotation in connection with the eastwardly moving currents in these latitudes, the velocities of which increase with increase of altitude very nearly in the same proportion as the steepness of the gradients. Although the steepness of these gradients at high altitudes, especially in the southern hemisphere, is considerable when considered with reference to gravity simply, yet, if all the forces are taken into account, there is no part of the atmosphere in the middle latitudes where the gradients are smaller, the velocity of the easterly motion being such as to not quite counteract the force from the gradients, and to leave a residual force simply which is sufficient to counteract the frictional resistance in these high altitudes, which is very small. It would be just as reasonable to maintain that there is a strong tendency in the water of the ocean to rush toward the poles, because there are steep gradients, considered with reference to the earth's attraction only, and leaving out of consideration that the centrifugal force arising from the earth's rotation counteracts this tendency, as to maintain that the air in these high altitudes has a strong tendency to rush toward the poles.

WM. FERREL.

Martinsburg, W. Va., Dec. 22.

Recent Investigation on the Causes of Cyclones and Anticyclones.

If I were required to name the man who impressed me as the most profound meteorological writer whom I had read, I should without hesitation say Professor Ferrel.

The most of us are qualitative meteorologists: he may be called a quantitative meteorologist. Not content with mere general statements of causes and forces, he attempts to determine the exact value of each one, and by rigid mathematical formulæ to determine if they are sufficient to account for the given results.

This represents a high, if not the highest, development of a scientific mind. For this reason I would hesitate to dissent from Professor Ferrel's conclusions more than from any writer I know; but he has himself, in his recent letter to *Science*, severely criticised the supposed blind following of authority, and, if there were needed any excuse, I would give this as the reason for presenting the views opposed to those of Ferrel.

There are two methods of arriving at results. The one is by deduction, in which the thinker, starting from axioms, well determined constants, or general laws, works out the results which must follow. The other is by induction, in which the thinker starts from observation, or separate individual facts, and arrives at general laws. Both methods are necessary; and most thinkers of to-day will admit that no theory of natural phenomena is complete until the results of deductive reasoning correspond to the results of inductive reasoning, or *vice versa*.

Now, Ferrel is essentially a deductive reasoner. It is necessary in such reasoning that the fundamentals, or physical constants from which one starts, should be correctly determined. In Ferrel's and Marvin's replies to Hazen in *Science* and in the *American Meteorological Journal*, I believe it has been shown that the constants forming the basis of the calculation in Ferrel's condensation theory of cyclones were satisfactorily determined. Starting with these, and following Espy, he has shown, that, given a warmer body of air, or a rapid vertical decrease of temperature over a considerable area, the causes are adequate to initiate and maintain a cyclone.

The question now is, do the investigations of inductive meteorologists sustain these views?

In order to study the results which follow rapid vertical decrease of temperature in the atmosphere, Loomis "selected from the volumes of the published observations of the Signal Service (November, 1873, to January, 1875, and from January, 1877, to May, 1877) all of the cases in which the temperature at Pike's Peak was 40° lower than at Denver." With this difference between them, the air would theoretically be in unstable equilibrium. "The number of these cases in twenty months of observation was 343. Only 39 of these cases occurred during the seven winter months of observation, and they occurred most frequently during the months of May. . . . The facts appear to show that at the dates given there were seldom any extraordinary disturbances on Pike's Peak. In two cases hail was reported, in four cases sleet and in fifteen cases either rain or snow. These facts seem to indicate an occasional uprising, but it is remarkable that so few such cases occurred; and it will be noticed that a difference of temperature of at least 45° between Pike's Peak and Denver often continued from day to day for long periods. . . . I think we may hence infer that dry air, even when greatly heated, has but little ascensional force" (Loomis's "Contributions to Meteorology," 13th paper, in *American Journal of Arts and Sciences*).

Loomis also found that heavy rainfall was not necessarily productive of cyclones. In his sixth paper, after examining a large number of cases, he says, "We conclude, therefore, that great rainfalls do not generally continue over eight hours, and very rarely do they continue for twenty-four hours, either as experienced at one station, or in succession at different places." He arrives at the same conclusion in his seventh and seventeenth papers, and adds, "The forces which impart that movement to the air which is requisite to an abundant precipitation of vapor, instead of deriving increased force from a great fall of rain, rapidly expend themselves, and become exhausted."

Furthermore, after examining a large number of areas of low barometric pressure with which there was little or no rain, he says, "There seems to be no room for doubt that barometric minima sometimes form with little or no rain, and continue without any considerable rain for eight hours, and sometimes for twenty-four hours or longer; . . . so that it seems safe to conclude that rainfall is not essential to the formation of areas of low barometer, and is not the principal cause of their formation or of their progressive motion."

"In order to determine the circumstances under which storms originate and ultimately acquire their full intensity," Loomis selected thirty-six cases from the Signal Service weather-maps in which the storm appeared to develop in the United States, and, as a result of a study of these, says, "The first stage in the development of each of these storms was an area several hundred miles in diameter, over which the height of the barometer differed but little from thirty inches, with an area of high barometer both on the east and west sides, and at a distance of about 1,000 miles. In the few cases in which a high barometer is not reported on both sides of the origin, it is because the area of observation is not sufficiently extended. The mean value of the barometer on the east side was 30.42 inches, and the mean distance 1.033 miles; on the west side the values were 30.31 inches and 977 miles. . . . On Hoffmeyer's storm-charts we frequently find three areas of high barometer surrounding an area of low barometer. These areas of high barometer are regarded as one of the causes, and generally the most important cause, of the storm which succeeds. . . . Since the air presses in on all sides towards this area of low barometer, the area tends to assume an oval form, which may become sensibly circular if the winds are very violent, and the centrifugal force resulting from this revolving motion causes a still further reduction of the barometer. . . . Rain is one of the circumstances which increases the force of a storm, and it invariably attends storms when they have attained considerable violence. . . . Some rain was invariably reported whenever the barometer fell below 29.4 inches, and generally there was some rain reported whenever the barometer fell below 29.5 inches. I have found no storm of great violence which was

not accompanied by a considerable fall of rain" (Loomis's eighth paper).

As early as 1876 Hann found, from the observations on the alpine peaks, that the highest temperature in the upper air occurred with the highest pressure, and explained it as due to the dynamic heating of descending air.

In 1886 Dechevrens showed that on the European peaks Pic du Midi and Puy de Dome, and on Pike's Peak in the United States, the lowest temperature occurred with the lowest pressure, which was exactly the opposite of observations at sea level. He also gave an example of simultaneous observations at the base and summit of the Puy de Dome during a low and during a high pressure, as shown by the barometer at both stations. At the base the temperature was highest with the low pressure, but at the summit the lowest pressure and temperature occurred together (*American Meteorological Journal*, August, 1886).

In the *American Meteorological Journal* for May, 1886, Mr. Dewey stated that from thirty-four pairs of observations during the winter months of 1872 and 1873 he found the average difference of temperature between Burlington, Vt., and the top of Mount Washington to be 6.6° F. when the latter was within a hundred miles of the centre of an anticyclone. The normal difference between the two stations is 19°. In the different quadrants of the anticyclone he found the following differences: north, 9°; east, 10°; south, 4.5°; west, 12.2°; average, 9°. He found the average difference two degrees greater in cyclones. Hazen's results for Mount Washington and Burlington, however, differ from these (*American Meteorological Journal*, October, 1887), so that further comparisons are needed.

In a footnote to an article on the origin and development of storms in the *American Meteorological Journal*, September, 1886, I cited the following reasons for thinking that warmer air is not the essential condition of storm-formation: "Storms sometimes originate along the eastern Rocky Mountain slope when the temperature of the air is lower there than in any part of the United States (for an example see the Signal Service charts of Jan. 19 and 20, 1886), and storms appear to originate in this region as often in the night as in the day."

Very recently Hann has investigated the temperature observations at numerous stations in the Alps during the passage of several cyclones (*Meteorologische Zeitschrift*, September, 1890), and has concluded that the temperature of the air-column as a whole is lower in cyclones than that of the surrounding air. Hann's investigations may not be conclusive for reasons stated by Ferrel, but they certainly add a link to the chain of evidence.

As a result of their investigations, Loomis and Hann both decided that cyclones were largely the result of mechanical causes. Loomis concluded that they were originated by the conflicting winds between two or more anticyclones, and Hann suggests that they are whirls originating in the upper air.

Now, I think Ferrel, in his recent letter to *Science*, unintentionally did Davis an injustice by suggesting that Davis had suddenly altered his opinion merely because Hann advanced these views. Davis has for years been the leading exponent in this country of the dynamical heating of the air in anticyclones, and during recent years I have several times spoken with him about the mechanical origin of cyclones; and, if he is now inclined to give these views more weight, it is because this last link in the chain of evidence has convinced him of the necessity of reconsidering the condensation theory.

I have for several years been convinced that mechanical action had much to do with the origin and development of cyclones, and as working hypotheses in making weather-predictions have carefully watched the following conditions as favorable for the production of cyclones: 1. The central region between approaching anticyclones. 2. The region where lower air-currents set in nearly opposed in direction to upper air-currents, so as to favor the production of a whirl. This latter condition is most frequently brought about in the United States when colder winds, moving from the north-west near the earth's surface, set in to the south or south-west of an area of high temperature or very high pressure, which give rise to upper currents moving from the south. This was the condition preceding the origin of the very violent storm

of March 12, 1888. 3. The deflection of air-currents by a long, tall range of mountains, such as the Rockies. I have several times predicted the origin of cyclones under these conditions. One of these was on April 19, 1883.

I have found the following conditions favorable to the increase of energy in cyclones: 1. The meeting of cyclones moving from nearly opposite directions; 2. The closing-up of a long trough of low pressure by the pressure increasing at both ends; 3. Cyclones, being mainly controlled in their movements by upper air-currents, are sometimes carried by these toward areas of denser air near the earth's surface, and under these conditions tend to increase in energy. Examples of violent storms, developed, as I think, by these mechanical methods, will be found on the following dates: Oct. 14, 1886; Jan. 9, 1889; and Jan. 9, 1886.

The immense gain that would come from being able to anticipate this class of storms may be inferred from the fact that not one of those I have mentioned in this paper was heralded by our Weather Service in time to be of any use, though the amount of damage done was enormous.

The views I hold are, that differences of pressure result from differences of temperature over immense areas, as between equator and pole, ocean and continent. This distribution of pressure is modified by the effect of the earth's rotation, and is continuously varying with the changes in temperature of the air.

The smaller cyclones and anticyclones of our weather-maps are partly or chiefly brought about by the mechanical action of counter-currents in the manner previously explained, though greatly modified by local differences of temperature and density within the cyclone: in other words, they are caused by forces originating outside their field of origin instead of within it, as supposed by Ferrel.

General rains are chiefly the result, and not the cause, of ascending currents of air. Differences of pressure in the upper air have a very important bearing on the origin and development of cyclones. Well-defined areas of low pressure, accompanied by precipitation and an inward tendency of the upper wind, occasionally exist in the upper atmosphere without being indicated by the barometric pressure at the earth's surface.

I have held most of these views for several years, as will be found by my review of Loomis in the *American Meteorological Journal*, and by two articles in *Nature* on the origin of anticyclones, and the cause of precipitation (*Nature*, vol. xxxvi. 1887, and vol. xxxviii. July, 1888), and have hoped to make some quantitative estimates of the forces and supposed causes; but I have not had the time, and fear I have not the ability to do so.

I trust Professor Ferrel will not dismiss these as vague hypotheses unworthy of notice, but will tell us (1) whether the method suggested by Loomis is insufficient to generate a cyclonic whirl according to mechanical principles; (2) whether conflicting air-currents can be supposed to have sufficient inertia to aid in producing a whirl, as, for instance, when denser air sets rapidly inward from both ends of a long trough of low pressure; and (3) whether such cyclones as that of Jan. 20, 1886, which originated near the longitude of Denver, where the temperature was lower than in any other part of the United States, when the observations on Pike's Peak showed no vertical decrease at all between the summit and base of the mountain, and when there was no appreciable precipitation within a thousand miles of the place of origin, could be explained by any reasonable assumption of a higher mean temperature of the air-column within the field of the cyclone.

H. HELM CLAYTON.

Blue Hill Observatory, Dec. 29.

BOOK-REVIEWS.

Handbook of Problems in Direct Fire. By JAMES M. INGALLS. New York, Wiley. 8°. \$4.

THIS book, which is believed to be the first of its kind ever published, shows the close attention now given to what may be called the scientific side of modern warfare, or, rather, of preparation for war. It is devoted wholly to problems in gunnery involving the use of ordinary service charges of powder and angles of elevation for the guns not exceeding 15°, which is the definition of

"direct fire." The author of the book, Capt. Ingalls of the First Regiment United States Artillery, instructor of ballistics at the United States Artillery School, has already given to the public two works on the same subject,—*"Exterior Ballistics,"* and *"Ballistic Machines."* This work was prepared while the author was engaged in teaching ballistics to student officers at the artillery school at Fort Monroe, and most of the examples are such as were given out from time to time to classes under his instruction, as exercises in ballistic formulæ. It will prove to be of permanent value, not only to the particular branch of the service for which it was intended, but also for other branches, both regular and militia. The most important of the examples may be worked out with a very slight knowledge of mathematics, arithmetic and a little algebra being sufficient for many of them.

AMONG THE PUBLISHERS.

IN *Lippincott's Magazine* for January, 1891, we note "The State of Washington," an article by Major Moses P. Handy, which will surprise the many who know little of this section of the country; and "The Road Movement," an article by Lewis M. Haupt, C.E., which contains some suggestions for the much-needed improvement of public roads.

—Messrs. Houghton, Mifflin, & Co. announce a new edition of Mr. Lowell's "Fable for Critics." This poem, in which all the prominent American authors of the period at which it was written are reviewed with keen appreciation mingled with good-natured banter, Mr. Lowell composed when he was under thirty years of age. "This *jeu d'esprit*," says Mr. Lowell in a prefatory note, "was extemporized, I may fairly say, so rapidly was it written, purely for my own amusement, and with no thought of publication. I sent daily instalments of it to a friend in New York, the late Charles F. Briggs. He urged me to let it be printed, and I at last consented to its anonymous publication. The secret was

kept till after several persons had laid claim to its authorship." There are twenty-six authors mentioned in the poem, and the publishers have made the book more interesting by securing portraits of each of these writers, taken about the time the original edition was published. These are reproduced in outline, and are inserted in the text at the point where each author is mentioned. A list of the authors alluded to is also given for the first time, so that the surmises to which the fable has always given rise will at last be set at rest.

—The first number of *The Bacteriological World*, edited by P. Paquin, M.D., Columbia, Mo., has appeared.

—A paper on the "Echinoderms from the Northern Coast of Yucatan and the Harbor of Vera Cruz," by J. E. Ives, assistant to the curator in charge of the Academy of Natural Sciences of Philadelphia, is published in the "Proceedings of the Academy of Natural Sciences of Philadelphia," Sept. 30, 1890. The *Echinodermata* which form the subject of this paper were collected on the northern coast of Yucatan and at Vera Cruz, in the spring of the present year, by an expedition from the Academy of Natural Sciences of Philadelphia to investigate the natural history of Yucatan and Mexico. The results in this department are interesting. One new genus and three new species are described, a little-known species is figured for the first time, the synonymy of this species and of some others has been studied with profitable results, and the majority of the species collected supply new localities which form connecting points between the northern and southern portions of the great West Indian, or eastern tropical American littoral fauna. The northern coast of Yucatan possesses a sandy beach largely made up of shell fragments. The water off the coast is very shallow, the ten-fathom line being twenty miles from the shore, and the hundred-fathom line about one hundred and fifty miles. Three miles off the shore in the neighborhood of Progreso, the bottom is of a sandy character, although

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a few small corals were brought up in the dredge. Along the shore to the westward of Progreso is a small serpuloid reef.

—Messrs. Ginn & Co. announce as ready "Quintus Curtius," the first two extant books, edited for sight-reading by Dr. Harold N. Fowler of Phillips Exeter Academy, with an introduction on reading at sight by Professor James B. Greenough of Harvard College. This book has been preferred on account of the conviction of the editor that for practice in sight-reading some continuous prose narrative not readily accessible in a copiously annotated edition should be in the hands of the pupil. The notes of this edition are confined to translations of unusual or striking words and phrases, with occasional brief hints concerning syntax, the main object of which is to save time in the class-room. In the introduction, Professor Greenough shows by examples the method to be pursued in reading at sight, besides explaining fully his ideas on the subject.

—The twenty-fifth volume of the *Magazine of American History* is opened with the January number. The leading illustrated paper for the month, from the pen of the editor, is entitled, "John Ericsson, the Builder of the 'Monitor,'" and a portrait of the inventor forms the frontispiece. The second article following, "The Bladensburg Duelling-Ground," near Washington, written by Milton T. Adkins, is also illustrated. The Georgia historian, Col. Charles C. Jones, jun., contributes a paper on "Dr. Lyman Hall, Governor of Georgia in 1783, and Signer of the Declaration of Independence;" Hon. Charles Aldrich of Iowa writes of the eloquence of Andrew Johnson; Hon. James Phinney Baxter, president of the Maine Historical Society, contributes "Isaac Jogues, A.D. 1636," a poem; Orrin B. Hallam gives the reader a history of the original treasury accounting office; and we have the first part of "Count de Fersen's Private Letters to his Father, 1780-1781," which are the observations and opinions of an officer under Rochambeau in the French Army during the Revolutionary

war, translated from the French by Miss Georgine Holmes. Among the Shorter papers, "The United States Flag," by J. Madison Drake, and "Capital Punishment in 1749," by Bauman L. Belden, are interesting.

—*The Monist* for January, 1891, a philosophical quarterly published by The Open Court Publishing Company, Chicago, contains "The Architecture of Theories," by Charles S. Peirce; "Illustrative Studies in Criminal Anthropology," by Professor Cesare Lombroso; "The Squaring of the Circle, the History of the Problem from the most Ancient Times to the Present Day," by Hermann Schubert; "The Criterion of Truth, a Dissertation on the Method of Verification," by Dr. Paul Carus; "Five Souls with but a Single Thought: the Psychology of the Star-Fish," by Carus Sterne; "German Philosophy in the Nineteenth Century," by Professor Friedrich Jodl; "Recent French Philosophical Works," by Lucien Arréat; book-reviews; and contents of the philosophical periodicals of America and Europe.

—"The fancy took me to go to Noto," says Mr. Percival Lowell, in his paper on "Noto: An Unexplored Corner of Japan," and where Noto is, and how he went there, is not only the subject of the opening article in the January *Atlantic*, but is to be the subject of several articles which are to follow. Cleveland Abbe's paper, which will command attention, suggests a new university course, this course to be devoted to terrestrial physics as a distinct department of instruction. Mr. Charles Worcester Clark writes about compulsory arbitration, in which he says that one of the most striking features of our easy-going American character is ready submission to the domination of our servants, whether it be Bridget in our kitchen, the railway in our streets, or Congress in the Capitol at Washington. Professor Royce has a long paper on Hegel, Adolphe Cohn writes about Boulangism, and Mr. Henry Charles Lea indicates the lesson of the Pennsylvania election.

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